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BEHAVIOR PATTERNS OF SCIENTISTS*

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I

The history of science indelibly records 1953 as the year in which the structure of the DNA molecule was discovered. But it is 1968 that will probably emerge as the year of the double helix in the history that treats the behavior of scientists, for James Watson's deeply personal account of that discovery has evidently seized the public imagination.

To judge from the popular reviews, the essential message of the book was taken to be: scientists are human, after all. This phrasing, it turns out, does not mean that scientists can be assigned at long last to the species *Homo sapiens*. Many Americans and some Englishmen were apparently prepared to entertain that serviceable hypothesis even before the appearance of *The Double Helix*. Evidently, what is meant by the Watson-induced thought that scientists, too, are human is that scientists are all too human.

What, then, are the stories Watson tells about the social and intellectual interactions that entered into the discovery, stories eliciting the popular response that scientists are all too human? Above all else, he tells of the race for priority; a close awareness of the champion rival who must be defeated in this contest of minds; a driving insistence on getting needed data from sometimes reluctant, sometimes inadvertent collaborators; a competition for specific discoveries over the years between the Cavendish and Caltech; an allegedly English sense of private domains for scientific investigation which bear no-poaching signs; an express ambition for that ultimate symbol of accomplishment, the Nobel Prize; he tells, too, about alternating periods of intense thought and almost calculated idleness (while the gestation of ideas pursues its course); about false starts and errors of inference; about quickly getting up needed scientific knowledge despite an impressive inventory of initial ignorance; about the complementarity of talents, skills, and character-structure of the symbiotic collaborators; about an unfailing sense for the key problem, and an intuitive and stubbornly maintained imagery of the nature of its solution, together with the implications as these were expressed in that master-stroke of calculated understatement wrought by Francis Crick: 'It has not escaped our notice that the pairing we have postulated immediately suggests a possible copying mechanism for the genetic material'.

The stories detailed in *The Double Helix* have evidently gone far to dispel a popular mythology about the complex behavior of scientists. That this response should have occurred among the public at large is not suprising. Embodying as they do some of the prime values of world civilization, scientists have long been placed on pedestals where they may have no wish to be perched—not, at least, the more thoughtful among them. This is not the result of a conspiracy, not even a conspiracy of good will. It is only that men and women of science have long been pictured, through collective acts of piety, as though they were more than human, being like gods in their creativity, and also as less than human, being deprived in their work of the passions, attitudes and social ties given to ordinary men. As a result, scientists have been dehumanized in the public mind by being idealized and, on occasion, idolized. Contributing greatly to this centuries-long process of distortion are the pious biographers who, in sapless prose, convert indubitably great men of science into what Augustus de Morgan once described as 'monsters of perfection'.

In part, too, the imagery of scientists moving coolly, methodically, and unerringly to the results they report may stem from the etiquette that governs the writing of scientific papers. That etiquette, as we know, requires them to be works of vast expurgation, stripping the complex events and behaviors that culminated in the report of everything except their cognitive substance. Compare only the lean, taut, almost laconic, 900-word article that appeared in *Nature* that momentous April in 1953 with the tangled web of events reported in Watson's 40,000-word account of the same discovery.

The sense of popular revelation upon learning that scientists are actually human testifies, then, to the prevalence of an earlier belief to the contrary. Ironically enough, that older mythology now threatens to be displaced by a somewhat new variant, expressed in responses to the Watson memoir by scientists and humanists alike. (I use the term mythology in its decidedly untechnical sense to denote a set of ill-founded beliefs held uncritically by an interested group.) The new variant has several interrelated components. The patterns of motives

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and behavior set out in Watson's irreverent, naturalistic narrative are held to be distinctive of the newest era of science staffed by 'a new kind of scientist and one that could hardly have been thought of before science became a mass occupation'. Only in our highly competitive age, allegedly, are appreciable numbers of scientists concerned to 'scoop' others at work in the field and so to gain recognition for their accomplishments. As another scientist-reviewer sees it, part of what Watson reports is an expression of 'no more than the general opportunism that is the hallmark of modern competitive science'—a statement in which the governing phrase is '*modern* competitive science'. And in still another version, this one the response of a humanist to *The Double Helix*, it is suggested, with unconcealed reluctance, that 'a keenness of early recognition may even be, these days, as essential to discovery as intelligence. Science, like all other activities *now*'—again, I accent the temporal qualifier—'is crowded and accelerated. There is no sitting alone *anymore* and letting apples fall down.'

There is a certain plausibility to this view that the mores of science and the behavior of scientists must surely have changed in the recent past. For plainly, all the basic demographic, social, economic, political, and organizational parameters of science have acquired dramatically new values. The size of the population of working scientists has increased exponentially from the scattered hundreds three centuries ago to the hundred or more myriads today. The time of the amateur is long since past; scientists are now professionals all, their work providing them with a livelihood and, for some, a not altogether impoverished one. The social organization of scientific inquiry has greatly changed, with collaboration and research teams the order of the day. As just another pale reflection of this altered organization of scientific inquiry, each decade registers more and more multi-authored articles in decided contrast to the almost unchanging character of single-authored papers in the humanities. The monumental budgets assigned to science—though never large enough, as all of us know—are orders of magnitude greater than the straitened budgets of only a few generations ago, to say nothing of the immense contrast with those of the more remote past. The vast increase in numbers of scientists and in funds for science practically dictates the exponential increase in the quantity of published research. As science has become more institutionalized, it has also become more intimately interrelated with the other institutions of society. Science-based technologies and the partial diffusion of a scientific outlook have become great social forces that move our history and greatly affect the relations obtaining between the nations of the world. Scientists do not, of course, make the major political decisions, but they now affect them significantly. The Szilard–Einstein letter to the President, for example, would be described by some as one of the most consequential communications in recorded history.

But it is not necessary to continue with this truncated list of particulars in which science today so conspicuously departs from the science of an earlier time. With all these profound changes, as almost any sociologist will tell you, if you give him half a chance, there must also be a new ethos of science abroad, a new set of values and institutionally patterned motives. And, as I have noted, practicing scientists in biology and physics and chemistry have indeed suggested that we now have a new breed of scientists, actuated by new motives, oriented to the main chance, and gravely agitated by failures to achieve. Like other men, scientists become disturbed by the pan-human problem of evil, in which, to assume the language of Gilbert Murray, 'the fortunes of men seem to bear practically no relation to their merits and efforts'.

Without at all adopting the new mythology of science, the psychiatrist Lawrence Kubie notes that young scientists, unwarned that 'their future success may be determined by forces which are outside their own creative capacity or their willingness to work hard "may suffer" a new psychosocial ailment . . . which may not be wholly related to the gangster tradition of dead-end kids.' And he goes on to ask: 'Are we witnessing the development of a generation of hardened, cynical, amoral, embittered, disillusioned young scientists?'

The question is not unrelated to the new mythology which maintains that behavior of the kind candidly described by Watson is something new to our time, and so, we must suppose, is altogether alien to the earlier, heroic age of science since, say, the seventeenth century. It is an intriguing and, as I have said, not altogether implausible thought, one which the rest of this paper is designed to examine.

II

As with most mythologies, this one is not altogether out of touch with the world of everyday experience. Though it may have surprised the outsider, Watson's unabashed report on the race for priority scarcely came as news to his fellow-scientists. They know from hard won experience that multiple independent discoveries at about the same time constitute one of their occupational hazards. They not only know it, but often act on that premise. That the consequent rush to achieve priority is common in our time hardly needs documentation.

On every side there is evidence that some unknown proportions of contemporary scientists are actively engaged in trying to get there first. But does the fact warrant the inference, drawn in the emerging mythology, that intense competition for discovery is in a significant sense distinctive of the new era of science, with its enlarged population of scientists, its grants, prizes, and professional rewards? I think not. This component of the mythology is the result of parochial perception. It emerges from the simple expedient of not looking at what there is to see throughout the centuries of modern science.

For the plain fact is, of course, that the race for priority has been frequent throughout the entire era of modern science. Moving back only a generation or so, we observe the good-natured race between Hahn and Boltwood, for example, to discover the 'parent of radium' which Boltwood was able to find first, just as, when Hahn discovered mesothorium, Boltwood acknowledged his having been outdistanced, saying only, 'I was almost there myself...' There is Ramsay telegraphing Berthelot in Paris 'at once' about his isolation of helium, writing Rayleigh to the same effect and sending a note to the Royal Society to establish priority, just as he and Travers were to announce having nosed out Dewar in the discovery of neon. There is the forthright account by Norbert Wiener of the race between Bouligand and himself in potential theory, making Wiener 'aware that he must hurry' but having it end in a 'dead heat' since Bouligand had submitted his 'results to the [French] Academy in a sealed envelope', just a day before Wiener had gotten off a short note for publication in the *Comptes Rendus* of the Academy.

In this respect the behavior of scientists does not much vary, transcending differences of time and national culture. Peter Kapitza puts it all in the of-course mood as he describes the behavior of Lomonosov, the father of Russian science, saying: 'No less importance was attached to priority in scientific work at that time than now'. Of this, Lomonosov and his colleagues provide dramatic evidence. When the physicist Richman was killed by lightning in 1763, the Russian Academy of Sciences canceled its general meeting, only to have Lomonosov ask that he nevertheless be given the opportunity to present his paper on electricity, 'lest', in his words, 'it lose novelty'. The President of the Academy saw the point and arranged for a special meeting in order, as he explained, 'that *gospodin* Lomonosov should not be late with his own new productions among scientific people in Europe, and his paper thereby be lost in electrical experiments made meanwhile'.

The fact is that almost all of those firmly placed in the pantheon of science—Newton, Descartes, Leibniz, Pascal, or Huygens, Lister, Faraday, Laplace, and Davy—were caught up in passionate efforts to achieve priority and to have it publicly registered. Consider only a highly condensed account how things stood with Newton. Now, I do not undertake to compare Newton and Watson in terms of their nature-given talents or their society-nurtured accomplishments. Such comparison would be not merely odious but downright foolish. But when we are told that the aggressive, prize-seeking, competitive and pathbreaking behavior of Watson is something new unleashed in the mid-twentieth century world of science, there is some point in examining the apposite behavior of the seventeenth-century giant of science. One incidental similarity of bare chronology is trivial enough to require no more than passing mention. They were both in their golden years decidedly young men. Just as Jim Watson took up the problem he made his own in his twenty-third year, so we will remember from Newton's own account, the *annus mirabilis* when at 23 or 24 he invented the binomial theorem, started work toward invention of the calculus, took his first steps toward establishing the law of universal gravitation, and began his experiments on optics.

Long after he had made these incomparable contributions to mathematics and physical science, Newton was still busily engaged in ensuring the lustre and fame owing him. He was not merely concerned with establishing his priority but was periodically obsessed by it. He developed a corps of young mathematicians and astronomers, such as Roger Cotes, David Gregory, William Whiston, John Keill and, above all, Edmond Halley, 'for the energetic building of his fame' (as the historian Frank Manuel has put it in his recent *Portrait of Isaac Newton*). Newton's voluminous manuscripts contain at least twelve versions of a defense of his priority, as against Leibniz, in the invention of the calculus. Toward the end, Newton, then president of the Royal Society, appointed a committee to adjudicate the rival claims of Leibniz and himself, packed the committee with his adherents, directed its every activity, anonymously wrote the preface for the second published report on the controversy—the draft is in his handwriting—and included in that preface a disarming reference to the legal adage that 'no one is a proper witness for himself and [that] he would be an iniquitous Judge, and would crush underfoot the laws of all the people, who would admit anyone as a witness in his own cause'. We can gauge the pressures for establishing his unique priority that must have operated for Newton to adopt such means for defense of his claims. As I shall presently suggest, this was not so much because Newton was weak as because the newly-institutionalized value set upon originality in science was so great that he found himself driven to these lengths.

By comparison, Watson's passing account of a priority-skirmish within the Cavendish itself can only be described as tame and evenhanded, almost magnanimous. That conflict largely testified to the ambiguous origins of ideas generated in the course of interactions between colleagues, touched, perhaps, with a bit of cryptomnesia.

Here, then, is one pattern that repeats itself through the centuries of modern science. Two or more scientists quietly announce a discovery. Since it is often the case that these are truly independent contributions, with each scientist having exhibited originality of mind, the process is sometimes stabilized at that point. But as the behavior of Newton, Leibniz, and an indefinitely large number of other scientists testifies, this peaceful acceptance of the fact of independent discovery does not always occur. Since the situation is often ambiguous with the role of each scientist not easy to identify and since each one knows that he had himself arrived at the discovery, and since the institutionalized stakes of reputation are high and the joy of acknowledged discovery immense, this militates against mutual acknowledgment of a parallel contribution. One or another of the discoverers—or, often, his colleagues or fellow nationals—suggests that he, rather than the rival, was really first, and that the independence of his rival is at least unproved. Then begins the familiar deterioration of standards governing conflictful interaction. The other side, grouping their forces, counter with the opinion that plagiary had indeed occurred, that let him whom the shoe fits wear it and, furthermore, to make matters quite clear, the shoe is on the other's foot. Reinforced by group loyalties and sometimes by ethnocentrism, the controversy gains force, mutual charges of plagiary abound, and there develops an atmosphere of thoroughgoing hostility and mutual distrust.

There is enough historical evidence, I suggest, to put into question the belief that science today is competitive to a degree unknown before. If there has been a change in this aspect of the ethos of science, it seems to be of quite another kind. Scientists have apparently become more fully aware that, with growing numbers at work in each special field, any discovery is apt to be made by others as well as themselves, and so are less often apt than before to assume that parallel discoveries must be borrowed ones. Among the multitude of multiple discoveries in the history of science, Elinor Barber and I have examined a sample of 264 in detail and have found, among other things, that there is a secular decline in the frequency with which multiples are an occasion for intense priority-conflicts. Of the 36 multiples before 1700 that we have examined 92 per cent were strenuously contested; the figure drops to 72 per cent in the eighteenth century; remains at about the same level in the first half of the nineteenth century and declines to 59 per cent in the second half, reaching the lowest level of 33 per cent in the first half of this century. Perhaps the culture of science today is not as pathogenic as it once was.

The absence of historical perspective marks another component of the new mythology of science. This one holds that quick, if not premature, publication to ensure priority is peculiar to our new breed of scientists, as witness the manuscript that went off to the editors of *Nature* on that fateful April 2 of 1953. Again, it will do no harm to examine this opinion from a sociological and historical perspective. Today as yesterday, scientists are caught up in one of the many ambivalent precepts contained in the institution of science. This one requires that the scientist must be ready to make his newfound knowledge available to his peers as soon as possible *but* he must avoid an undue tendency to rush into print. To see this in fitting historical context, we must remember that the first scientific journals confronted not an excess but a deficiency of manuscripts meriting publication. The problem did not arise merely from the small number of men at work in science. There was the further restraint that the value set upon the open disclosure of one's scientific work was far from universally accepted. Intent upon safeguarding their intellectual property, many men of science in the seventeenth century set a premium upon secrecy (as is evident from their correspondence with close associates).

To convert this motivated secrecy into motivated free disclosure, Henry Oldenburg, the first editor of the *Philosophical Transactions*, introduced an expedient for maintaining property rights through prompt publication. In this way, the contributor would be assured his priority. We see the effectiveness of this socially patterned motivation beautifully exemplified in the case of Robert Boyle, who, like others of his time, was chronically and acutely anxious about the danger of what he described as 'philosophical robbery', what would be less picturesquely described today as pilfering from circulated but unpublished manuscripts. Boyle felt that he had often been so victimized. But now, the editor Oldenburg could assure Boyle and others that their priority rights would be guarded as never before. Exceedingly prompt publication in the *Transactions* would take care of that. As Oldenburg wrote Boyle about his perennially 'lost papers': 'They are now very safe, and will be within this week in print, as [the printer] Mr. Crook assureth, who will also take care of keeping ym unexposed to ye eye of a Philosophical Robber'. Thus, from its very beginning, the journal of science introduced the institutional device of quick publication to motivate men of science to replace the value set upon secrecy with the value placed upon the open disclosure of the knowledge they had created. The concern with getting into print fast is scarcely confined to contemporary science.

Watson fluttered the dovecotes of academia, to say nothing of the wider reading public, by telling us of having joined with Crick in an enthusiastic toast 'to the Pauling failure, . . . Though the odds still appeared against us, Linus had not yet won his Nobel.' Once again, it seems, Watson had violated the mores that govern contest behavior in science and the public disclosure of that behavior. Yet seen in historical perspective, how mild and restrained is this episode by comparison with judgments on contemporaries set out in public by great scientists of the heroic past. There is Galileo becoming a seasoned campaigner as he flays one Grassi who 'tried to diminish whatever praise there may be in this [invention of the telescope] which belongs to me'. Galileo then goes on to assail others who, 'attempted to rob me of that glory which was mine, pretending not to have seen my writings and trying to represent themselves as the original discoverer of these marvels'. Or finally, Galileo says of a third that he 'had the gall to claim that he had observed the Medicean planets . . . . before I had [and used] a sly way of attempting to establish his priority'.

As we approach our own day, we hear only a muted echo of these angry and agitated words reverberating through the corridors of the peaceful temple of science. Since some of these episodes involve our contemporaries and often our associates, they become, we must suppose, painful to observe and more difficult to analyze with detachment. Even the social scientists who may not be directly involved in these episodes, at least for the moment, feel acutely uncomfortable. Uneasy and distressed, they can hardly bring themselves to study this behavior. Even to assemble the facts of the case is to be charged with blemishing the record of undeniably great men of science as though one were a raker of muck that a gentleman would pass by in silence. Even more, to investigate the subject systematically is to be regarded not merely as a muck-raker, but as a muck-maker. For when sociological analysis is stripped bare of sentiment, it often leaves the sociologist shivering in the cold. And to respond with detachment to these hot conflicts becomes all the more difficult. So, though historical facts to the contrary are abundantly available, there emerges a new mythology that treats competitive behavior of scientists as peculiar to our own competitive age.

III

This introduces an instructive paradox. These, indeed, are changing times in the ethos of science. But Watson's brash memoir does not testify to a breakdown of once prevailing norms that call for discreet and soft-spoken comment on scientific contemporaries. A memoir such as his would have been regarded as a benign model of disciplined restraint by the turbulent scientific community of the seventeenth century. That it should have created the stir it did testifies that, with the institutionalization of science, the austere mores governing the public demeanor of scientists and the public evaluation of contemporaries have become more exacting rather than less. As a result, Watson's little book, so restrained in substance and so mild in tone by comparison with the caustic and sometimes venomous language of, say, Galileo or Newton, violates the sentiments of the many oriented to these more exacting mores.

Within such a context, the behavior of scientists involved in races for priority or in the increasingly rare disputes over priority tends to be condemned, rather than analyzed. It is morally judged, not systematically investigated. The disputes are described as 'unfortunate' with the moral judgment being substituted for the effort to understand what they imply for the psychology of scientists and the sociology of science as an institution. At least since Goethe, we note references to 'all those foolish quarrels about earlier and later discovery, plagiary, quasi-purloinings'. We are free, of course, to find this behavior unfortunate or foolish or comic or sad. But these affective responses to the behavior of our ancestors-in-science or our brothers-in-science have usurped the place that might be given to analysis of this behavior and its implications for the ways in which science develops. It is as though the physician were to respond only evaluatively to illness, describe it as unfortunate or painful and consider his job done, or as though the psychiatrist were to describe the behavior of schizophrenics as absurd and to substitute this sentiment for the effort to discover what brings that behavior about. The undisciplined tendency to respond in terms of sentiments has generated resistance to recognizing the central role of competition throughout the modern era of science.

This resistance is expressed in various ways: by seeking to trivialize the fact, by regarding the concern with priority as rare or aberrant (when it is in truth frequent and typical), by motivated misperceptions of the facts or by an hiatus in recall and reporting. Such resistance often leads to those wish-fulfilling beliefs, false memories, and mythologies that we describe as illusions. And of such expressions of resistance the annals of science are uncommonly full. So much so, that I have arrived at a rule-of-thumb that seems to work fairly well. The rule is this: whenever the biography or autobiography of a scientist announces that he had little or no concern with priority, there is a reasonably good chance that not many pages later in the book, we shall find him deeply embroiled in one or another episode where priority is at issue.

For example, the authoritative biography of that great psychiatrist of the Salpêtrière, Charcot, states that, despite his many discoveries, he 'never thought for a moment to claim priority or reward'. Our rule of thumb leads us to expect what we find: some thirty pages later, there is a detailed account of Charcot insisting on having been first in recognizing exophthalmic goiter and a little later, emphatically affirming that he 'would like to claim priority' (the language is his) for the idea of isolating patients suffering from hysteria.

Or again, Harvey Cushing writes of the brilliant Halsted that he was 'over-modest about his work, indifferent to matters of priority'. Alerted by our rule of thumb, we find some twenty pages later in the book where this is cited, a letter by Halsted about his work on cocaine: 'I anticipated all of Schleich's work by about six years (or five) . . . I showed Wölfler how to use cocaine. He had declared that it was useless in surgery. But before I left Vienna he had published an enthusiastic article in one of the daily papers on the subject. It did not, however, occur to him to mention my name'.

Not only the historians and biographers of science but scientists themselves often manifest ambivalence toward the facts of priority-oriented behavior. Even while he was assembling documents to prove his priority, for example, Darwin registers his mixed feelings, writing Lyell: 'My good friend, forgive me. This is a trumpery letter, influenced by trumpery feelings.' In a postscript, he assures Lyell that 'I will never trouble you or Hooker on the subject again.' The next day, he writes Lyell: 'It seems hard on me that I should lose my priority of many years' standing'. Then, a few days later, he writes again to say: 'Do not waste much time [on this matter]. It is miserable in me to care at all about priority'. Moreover, we need not have waited for the Watson memoir to be reminded that different styles of scientific investigation are variously bound up with different roles in achieving priority. Fifty years after the joint Darwin–Wallace paper was presented to the Linnean Society, Wallace was still insisting upon the contrast between his own hurried work, written within a week after the great idea came to him, and Darwin's work, based on twenty years of collecting evidence. 'I was then (as often since) the "young man in a hurry",' said the reminiscing Wallace, '*he*, the painstaking and patient student seeking ever the full demonstration of the truth he had discovered, rather than to achieve immediate personal fame'.

Freud recognizes his own ambivalence when he writes of his work on the 'Moses' of Michelangelo that, having come upon a little book published in 1863 by an Englishman, Watkiss Lloyd, he read it: 'with mixed feelings. I once more had occasion to experience in myself what unworthy and puerile motives enter into our thoughts and acts even in a serious cause. [Take note of the language: 'unworthy and puerile motives', for we shall be returning to their implications before long]. My first feeling was of regret that the author should have anticipated so much of my thought, which seemed precious to me because it was the result of my own efforts; and it was only in the second instance that I was able to get pleasure from its unexpected confirmation of my opinion. Our views, however, diverge on one very important point'.

This degree of self-awareness is a far cry from the ambivalence of a Descartes who manages to write that 'he does not boast of being the first discoverer' and then proceeds to insist on his priority over Pascal or to beg his friend Mersenne 'to tell him [Hobbes] as little as possible about . . . my unpublished opinions, for if I'm not greatly mistaken, he is a man who is seeking to acquire a reputation at my expense and through shady practices'.

IV

All of this brings us finally to the question touched off by the responses of many scientists and laymen to the Watson memoir. We are now perhaps ready to see that those responses relate to the long-standing denial that, through the centuries, scientists, and often the greatest among them, have been concerned with achieving and safeguarding their priority. The question is, of course: what leads to this uneasiness about acknowledging the drive for priority in science? Why the curious notion that a thirst for significant originality and for having that originality accredited by competent colleagues is depraved?

In one aspect, the embarrassed attitude of a Darwin or Freud toward their own interest in priority is based upon the implicit assumption that behavior is actuated by a single motive, which can then be appraised as good or bad, as noble or ignoble. It is assumed that the truly dedicated scientist must be moved only by the concern with advancing knowledge. As a result, deep interest in having his priority recognized is seen as marring his nobility of purpose as a man of science (although it might be remembered that 'noble' once meant the widely-known). The assumption of a single motive is of course unsound. Scientific inquiry, like human action generally, stems from a variety and amalgam of motives in which the passion for creating new knowledge is supported by the passion for recognition by peers and the derivative competition for place.

There is, nevertheless, a germ of psychological truth in the suspicion enveloping the drive for recognition in science. Any extrinsic reward—fame, money, position—is morally ambiguous and potentially subversive of culturally esteemed values. For as rewards are meted out, they can displace the original motive: concern with recognition can displace concern with advancing knowledge. An excess of incentives can produce distracting conflict. But when the institution of science works effectively, and like other social institutions it does not always do so, recognition and esteem accrue to those scientists who have best fulfilled their roles, to those who have made the fundamental contributions to the common stock of knowledge. Then are found those happy circumstances in which moral obligation and self-interest coincide and fuse. The ambivalence of scientists toward their own interest in having their priority recognized—an ambivalence we have seen registered even by that most astute of psychologists, Freud—shows them to assume that such an ancillary motive somehow tarnishes the purity of their interest in scientific inquiry. Yet it need not be that scientists seek only to win the applause of their peers but, rather, that they are comforted, reassured and gratified by it when it does ring out. In the rare instance, they may even catch a glimpse of their own immortality.

In another aspect, the ambivalence toward priority means that scientists reflect in themselves the ambivalence built into the social institution of science itself. On one side, the institutional norms of science exert pressure upon scientists to assert their claims and this goes far toward explaining the seeming paradox that even those meek and unaggressive men, such as Henry Cavendish and James Watt in The Water Controversy, ordinarily slow to press their claims in other spheres, will often do so in their scientific life. The ways in which the norms of science help produce this result seem clear enough. On every side, the scientist is reminded that it is his role to advance knowledge and his happiest fulfillment of that role to advance knowledge greatly. This is only to say, of course, that, in the institution of science, originality is at a premium. For it is through original contributions, in greater or smaller increments, that knowledge advances. Having acquired this sentiment from the institution of science, scientists find it difficult to give up a claim to a new idea or a new finding which in effect testifies to others and to themselves that they have lived up to their commitment. Yet the same institution of science emphasizes selfless dedication to the advancement of knowledge. Concern with achieving priority and ambivalence toward that concern register in the individual scientist what is generated by the complex value-system of science.

In still another aspect, ambivalence toward concern with priority derives from the mistaken belief that it must express naked self-interest, that it is altogether self-serving. On the surface, the hunger for recognition appears as mere personal vanity, generated from within and craving satisfaction from without. But when we reach deeper into the institutional complex that gives added edge to that hunger, it turns out to be anything but personal, repeated as it is with slight variation by one scientist after another. Vanity, so-called, is then seen as the outer face of the inner need for assurance that one's work really matters, that one has measured up to the hard standards maintained by at least some members of the community of scientists. Sometimes, of course, the desire for recognition is stepped up until it gets out of hand. It becomes a driving lust for acclaim; megalomania replaces the comfort of reassurance. But the extreme case need not be taken for the modal one. In providing apt recognition for accomplishment, the institution of science serves several functions, both for men of science and maintenance of the institution itself.

V

Thus the community of science provides for the social validation of scientific work. In this respect, it amplifies that famous opening line of Aristotle's *Metaphysics:* 'All men by nature desire to know.' For men of science by culture desire to know that what they know is really so. The organization of science operates as a system of institutionalized vigilance, involving competitive cooperation. It affords both commitment and reward for finding where others have erred or have stopped before tracking down the implications of their results or have passed over in their work what is there to be seen by the fresh eye of another. In such a system, scientists are at the ready to pick apart and assess each new claim to knowledge. This unending exchange of critical appraisal, of praise and punishment, is developed in science to a degree that makes the monitoring of children's behavior by their parents seem little more than child's play. Only after the originality and consequence of his work have been attested by significant others can the scientist feel reasonably confident about it. Deep felt praise for work well done, moreover, exalts donor and recipient alike; it joins them both in symbolizing the common enterprise. That, in part expresses the character of competitive cooperation in science.

The function of reassurance by recognition has a dependable basis in the social aspects of knowledge. Few scientists have great certainty about the worth of their work. Even that psychological stalwart, T. H. Huxley, seemingly the acme of self-confidence, tells in his diary what it meant to him to be elected to the Royal Society at the age of 26, by far the youngest in his cohort. It provided him, above all, with much needed reassurance that he was on the right track; in his own language, 'acknowledgement of the value of what' he had done. And since, like the rest of us, Huxley was occasionally inclined to doubt his own capacities and to think himself a fool, he concluded that 'the only use of honours is as an antidote to such fits of "the blue devils".'

But authentic reassurance can be provided only by the scientists whose judgment one in turn respects. As we sociologists like to put it, we each have our reference groups and individuals, whose opinions of our performance matter. Our peers and superiors in the hierarchy of accomplishment become the significant judges for us. Darwin writing Huxley about the *Origin of Species* 'with awful misgivings' thought that 'perhaps I had deluded myself like so many have done, and I then fixed in my mind three judges, on whose decision I determined mentally to abide. The judges were Lyell, Hooker, and yourself'. In this, Darwin was replicating the behavior of many another scientist, both before and after him.

Other strategic facts show the inadequacy of treating an interest in recognition of scientific work as merely an expression of egotism. Very often, the discoverers themselves take no part in arguing their claims to the priority of significance of their contributions. Instead, their friends or other more detached scientists see the assignment of priority as a moral issue not to be scanted. For them, the assigning of all credit due is a functional requirement for the institution of science itself. After all, to protect the priority of another is only to act in accord with the norm, which has been gathering force since the time of Francis Bacon, that requires scientists to acknowledge their indebtedness to the antecedent work of others.

Now these bystanders stand to gain little or nothing from advancing the claims of their candidates, except in the Pickwickian sense of having identified themselves with them. Their behavior can scarcely be explained by egotism. Their own status is not being threatened. Instead, their disinterested moral indignation is a signpost announcing the violation of a moral norm in the institution of science. In this sense, the concern with priority, with all the passionate feelings it often evokes, is not merely an expression of self-interest or hot tempers, although these may raise the temperature of controversy. Rather, they constitute responses to the institutional norms of intellectual property, norms that transcend the personality needs of this or that scientist.

From still another perspective we can see the fallacy of the new mythology that construes the thirst for priority as altogether self-serving. Often the drive for recognized originality is only the other side of the coin of the elation that comes from having arrived at a new and true scientific idea or result. The deeper the commitment to the discovery, the greater, presumably, the reaction to the threat of having its originality denied. Concern with priority is often the counterpart to elation in discovery—the Eureka syndrome. We have only to note Kepler's ecstatic expression of joy on his discovery of the third planetary law.

In short, when a scientist has made a discovery that matters, he is as happy as a scientist can be. But the height of exultation may only deepen the plunge into despair should the discovery be taken from him. If the loss is occasioned by finding that it was, in truth, not a first but a later independent discovery, that he had lost the race, the blow may be severe enough, though mitigated by the sad consolation that at least the discovery had been confirmed by another. But this is nothing, of course, when compared with the traumatizing experience of having it suggested that the discovery was not only later than another of like kind but that it was really borrowed. The drive for priority is in part an effort to reassure oneself of a capacity for original thought. Thus, rather than being mutually exclusive, as the new mythology of science would have it, joy in discovery and the quest for recognition by scientific peers are stamped out of the same psychological coin. In their conjoint ways, they both express a basic commitment to the value of advancing knowledge.

Chargaff is correct, I believe, in suggesting that the Watson memoir 'may contribute to the much-needed demythologizing of modern science'. But as I have tried to suggest, to put the accent on '*modern* science' is only to displace the old myth with a new variant. In noting this, I am scarcely alone. Some practicing scientists, both before and after *The Double Helix*, have put aside the myth that competition for originality in science is alien to joy in discovery and that the drive for recognition should occasion self-contempt. Hans Selye asks his peers: 'Why is everybody so anxious to deny that he works for recognition? . . . all the scientists I know sufficiently well to judge (and I include myself in this group) are extremely anxious to have their work recognized and approved by others. Is it not below the dignity of an objective scientific mind to permit such a distortion of his true motives? Besides, what is there to be ashamed of?' And, as though he were responding to this rhetorical question, P. B. Medawar goes on to argue: 'In my opinion the idea that scientists ought to be indifferent to matters of priority is simply humbug. Scientists are entitled to be proud of their accomplishments, and what accomplishments can they call "theirs" except the things they have done or thought of first? People who criticize scientists for wanting to enjoy the satisfaction of intellectual ownership are confusing possessiveness with pride of possession'.

Both the practicing scientist and the practicing poet perceive the deeper implications of the thrust for significant and acknowledged originality in living science. With the poet's inward eye, Robert Frost puts it so.*

Would he mind had I
Had him beaten to it?
Could he tell me why
Be original?
Why was it so very,
Very necessary
To be first of all?
How about the lie
Someone else was first?
He saw I was daffing.
He took this from me.
Still it was no laughing
Matter I could see.
He made no reply.

Of all crimes the worst
Is the theft of glory,
Even more accursed
Than to rob the grave.

The history of science declares what the poet sings: a care for truth signifies a care for the truth-seeker.
